beetle trap. That it eats caterpillars is also certain: but it feeds more especially on the larger insects, such as may-bugs, dung-beetles, large night-flying moths, especially the Sphinx Moth, and various species of nocturnal insects. It is a very greedy feeder, and in the autumn is often very fat. The indigestible portions of the insects it devours (which it swallows entire) it throws up in long pellets, which may frequently be found in the places where it reposes during the day. As it feeds more especially on those insects which are to be met with amongst the dung in places where cattle have been feeding, or where they are stalled, the Night-jar is often to be met with in these pastures or in the immediate vicinity of outlying folds; and hence the popular delusion that it sucks the goats hanging on to their udders; and from this belief has arisen the common appellation of Goat-sucker.

"This species has the claw of the middle toe furnished on the side with pectinations forming a sort of close-toothed comb; and the use made of this peculiar appendage has puzzled naturalists not a little. Some observers contend that it is used to clean the bristles at the base of the bill from the fragments of wings of insects which may adhere to them; but this cannot well be the case, as these vibrissæ or bristles are large, strong, and placed at some distance apart, whereas the teeth of the claw are thin and very close. Others think that as the bird invariably perches along a branch in a direction parellel with it, and never across the bough like almost all other birds, this pectinated claw may assist it in keeping its perch more firmly than it otherwise would do. Other naturalists, again, contend that it is used to hold large insects with greater security; but it appears that the Night-jar almost invariably takes its prey with the mouth and not with the foot; and consequently this supposition falls to the ground. An anonymous writer suggests that the comb-like structure of the claw may be used for disengaging the hooked feet of beetles from the bill, to enable the bird to swallow them; and this may possibly be the case, as the serrations are well calculated to catch the polished limbs of beetles. Anyone who has attempted to confine *Dytisci* or *Scarabæi* in a collecting-box, must be aware of the difficulty in getting their feet free from the edge, to which they hold with the greatest pertinacity, one foot being no sooner pushed in than another is protruded."

This last explanation seems the most probable one, and it agrees with the observation of Gilbert White (of Selborne), who states that he has distinctly seen the Night-jar raise its foot to its mouth while hawking for insects on the wing.

The passage above quoted is a portion of seven quarto pages devoted to an account of the habits and distribution of the Night-jar. A work like the present, so beautifully and artistically illustrated, and of which only a limited number of copies is printed, is sure to become scarce and to rise considerably in value. Lovers of nature and of art may therefore be reminded, that in becoming subscribers they are not only obtaining a valuable and most interesting book, but are at the same time making a profitable investment. A. R. W.

OUR BOOK SHELF

The Monthly Journal of Education and Scholastic Advertiser. A medium of intercommunication for Masters, Mistresses, and others interested in Education. Nos. 1 to 16. (W. P. Nimmo, 1874, 1875.)

THE original *Quarterly* form of this journal had been for some years "slowly but steadily increasing in circulation." The journal is now issued as a *monthly* publication "by a number of teachers who are anxious to be of service to their fellow-workers, and to all persons interested in education." The editor and principal contributors to the two forms of the journal being the same, as might be expected there is no great difference in the earlier and later volumes, but yet there is, we believe, an improvement on the side of the present series. The advantage of such a frequent issue is pretty obvious, but the meeting the subscription for twelve numbers instead of four, is to some a serious consideration. The number of subscribers, we find, is fairly satisfactory, but to make it more than a barely paying matter a much larger number of subscribers, the editor states, is required.

Glancing rapidly over the articles in the numbers before us, we just indicate a few which strike us as most generally interesting. The first we light upon is a letter from Mr. Wilson, of Rugby, to Dr. Temple, on Successive *v.* Simultaneous Instruction: it was written in January 1869, and in considering the problem of education advocates the "stratification of studies." The question is naturally discussed with an eye to Rugby, but the paper is, as might be supposed, deserving of careful study by outsiders. Another Rugby master, Mr. Kitchener, gives his views on teaching botany to junior classes; and Mr. J. Clifton Ward on natural science teaching in schools. A paper on trifle blindness advocates Dr. Liebrich's views. Besides, we note a reprint of a paper by Dr. Hodgson, on exaggerated estimates of reading and writing; one on French accent; and one, by Dr. Jones, on Mr. Todhunter's essay on Elementary Geometry. These two should be read by all who may wish to see what can be said for and against Euclid as a school text-book of geometry. A portion of each number is devoted to correspondence, and a new feature in this new issue of the journal is a Mathematical Column. What the journal wants is the support and contributions of more of our foremost educationalists, and then it would take a higher position than it does at present.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

On the Dynamical Evidence of Molecular Constitution

I BEG to offer the following remarks upon the extremely valuable and instructive lecture by Prof. Clerk-Maxwell which appeared in NATURE, vol. xi. pp. 357, 374, in the hope that they may tend to the further elucidation of this interesting subject.

If two bodies are attracted towards each other by a force which varies inversely as the square of the distance, and R, r, be the force and distance at any instant, $R r$ will represent the sum of that portion of the energy of the two bodies which is due to their mutual attraction (the mean being $\frac{1}{2} R r$); that is, the amount which would be converted from potential to actual energy while they approached each other to this point from an infinite distance.

The sum of the virials $\Sigma\Sigma(\frac{1}{2} R r)$, or $\Sigma(R r)$, will therefore represent, for a gas whose molecules are so attracted, the total amount of the energy due to attraction.

According, therefore, to the formula of Clausius, the elasticity of such a gas would be the same as if those forces and a portion of the kinetic energy of translation of every particle equal to the energy which is due to them had no existence.

And as the distances between the particles vary inversely as the cube root of the density, if the attractive forces vary inversely as the square of the distances, $\Sigma\Sigma(\frac{1}{2} R r)$ will vary directly as the cube root of the density. The deduction from the element of $p V$ represented by $\frac{2}{3} T$ will therefore vary as the cube root of the density, and the value of $p V$ will diminish as the density increases.

If the attractive forces vary in a higher inverse ratio, this effect will be further increased.

And if this ratio be the nth power, the sum of the virials wi

be the energy due to the attraction of those forces multiplied by $n - 1$, and for a given quantity of gas will vary as the density raised to the power of $\frac{n-1}{3}$.

The sum of the virials due to gravitation does not appear sufficient to account for the observed effects, and moreover would vary with the quantity of gas in a compound ratio. We must conclude, then, that the ratio of the force to the distance is a higher one than that of the inverse squares.

Upon that law, as already stated, the sum of the virials would increase, for equal quantities of gas, in the ratio of the cube root of the density. Prof. Maxwell has shown that for equal volumes the increase must be as the square of the density, that is, for equal quantities as the density. In order to obtain the same result directly, supposing the density to vary, the quantity remaining constant, it is necessary to assume the forces to vary inversely with the fourth power of the distances. On this supposition the sum of the virials will vary for a given density, as the facts appear to indicate, directly with the volume.

The formula of Clausius does not elucidate the phenomenon of the increase of $p\,V$ at low densities with increase of density, experimentally demonstrated in the case of hydrogen gas only, but probably true, as conjectured by Regnault, of other gases also at sufficiently high temperatures.

The *rationale* of this I believe I have discovered, but will not now attempt to enter upon this point.

Prof. Maxwell mentions that Clausius had long ago pointed out that the ratio of the increment of the whole energy to that of the energy of translation may be determined if we know by experiment the ratio of the specific heat at constant pressure to that at constant volume.

The same result is obtained by comparing the specific heat at constant volume with the difference in the kinetic energy of translation on increase of temperature indicated by the increase of pressure; a method by which a small error arising from the variation in the value of $p\,V$ at different densities is eliminated, the sum of the virials remaining constant.

Taking c_1 to represent the specific heat at constant volume, J the mechanical equivalent of heat, p and V the initial pressure in pounds per square foot and volume in feet of a pound weight of the gas, T_0 and T_1 the energy exclusive of that of translation at zero and 1° Centigrade respectively, α the coefficient of expansion for constant volume,* we shall have—

$$c_1 J - \tfrac{3}{2}\,\alpha p\,V = T_1 - T_0.$$

For atmospheric air $c_1 J$ may be taken at 233·41, and $p\,V$ at 26215, and α, by Regnault's experiments, is ·003665, so that—

$$T_1 - T_0 = 233{\cdot}41 - 144{\cdot}12 = 89{\cdot}29.$$

This gives the increment of energy due to other motions than that of translation not quite two-thirds of that due to the motion of translation. The exact ratio is 1·859 to 3.

The experiments of Regnault prove that neither $p\,V$ nor α are absolutely constant at all densities. He found α at ·1444 atmosphere to be ·0036482 and 4·81 atmosphere ·0037091. His experiments do not indicate an appreciable difference in the value of $p\,V$ between ·1444 and 1 atmosphere, but between 1 and 4·81 atmosphere it appears to be diminished about ·004 of its amount. The value of $\frac{3}{2}\,\alpha p\,V$ in the former case will therefore be about 143·46, and in the latter 145·27.

Supposing the specific heat to be independent of density, this would indicate that the ratio of the increment of the energy of translation to that of the remaining energy, and therefore probably that of the energies themselves, increases with the density. It is, however, not improbable that c_1 may likewise vary, and that the ratio of the two elements may be constant.

M. Regnault's experiments to determine the specific heat of air were all made at somewhat high pressures, varying at the commencement of the experiments from 4 to 6000 mm., and at the termination from 800 to 3000 mm. They more nearly correspond, therefore, to a pressure of 4·81 atmosphere than to 1 atmosphere. And if $\frac{3}{2}\,\alpha p\,V = 145{\cdot}27$, $T_1 - T_0 = 88{\cdot}14$, a ratio between the elements of the energy of 3 to 1·82.

It is also probable that c_1 varies to some extent at different temperatures, but I am not aware that any experiments have been made to ascertain this. Regnault throughout assumes the specific heat to be constant for all temperatures.

Prof. Maxwell states that a consequence of Dr. Boltzmann's theorem is that the temperature tends to become equal throughout a vertical column of gas at rest. He also confirms this doctrine as an independent conclusion of his own.

It is with great diffidence that I advance a different view from that which has the sanction of such high authority.

It seems obvious, however, that the mean energy of the molecules moving downwards must be increased, and that of those moving upwards diminished, by the amount of the work of gravitation. And there is nothing to counteract this tendency unless there is repulsion between the particles; attraction would increase it. At all parts of the system there is exchange of energy between the particles; but, supposing equilibrium to have been attained, the mean amounts of energy transmitted in opposite directions at any given point must be equal. Equilibrium, therefore, can only exist when the difference of the actual energy at different distances from the centre of attraction is the same as the difference due to the transfer of particles from one distance to the other.

I think that the equality of temperature must be involved, either explicitly or implicitly, in the data from which the theorem of Boltzmann is deduced.

If this reasoning is correct (supposing the gas at rest), the following equation will represent the relation of the temperatures at different elevations:—

$$t_1 = t_2 - \frac{x_1 - x_2}{c_1 J}$$

where $x_1\ x_2$ are the heights, $t_1\ t_2$ the corresponding temperatures.

The difference of temperature which would exist in the atmosphere at different heights in consequence of this law (one degree Centigrade for every 233 feet) is partly counteracted by the action of the currents; the rate of cooling by expansion being less for the same difference of height. But in a long-continued calm the increase of heat in the lower region of the atmosphere is well known to be intense.

If the condition of equal temperature at all heights were one of atmospheric equilibrium, it would be one of stable equilibrium. It would sooner or later be attained, and would be subject to little disturbance. But the equilibrium which the law above stated tends to induce in still air is extremely unstable, inasmuch as a body of air which has risen in consequence of being warmer and lighter than the surrounding portions of the same stratum has a still greater difference of temperature from the higher strata, having suffered less refrigeration from expansion than that due to the difference of elevation in still air. Slight affections of temperature are therefore capable of causing great atmospheric disturbances, and the tropical calms before alluded to are commonly followed by the most violent tempests.

Prof. Maxwell observes that a molecular æther would be neither more nor less than a gas. This statement requires one qualification, as the theory does not necessarily imply the existence of either attraction or repulsion between the particles, and from the universal diffusion of the æther it must be inferred that no such forces exist. This constitutes a difference of some importance from the condition of a gas. It is true that an equilibrium of temperature would *tend* to establish itself between the agitation of the ordinary molecules and those of the æther. But the establishment of such an equilibrium would be constantly counteracted by the rapid transmission of the energy communicated to them, through space, by the molecules of the æther; in other words, by *radiation*. There are doubtless difficulties in this hypothesis, but its rejection involves the conception of the transmission of energy by other means than the motion of material particles, and we have no sufficient ground for supposing any other mode of transmission to be possible.

It has been suggested that the alternative to the conception of a molecular æther is a continuous material substance, not made up of parts, and that such a substance might be capable of motion and of transmitting energy. But a continuous material substance, not made up of separate parts, capable of internal motion, and permeable throughout by ordinary matter, can hardly be called material in an ordinary sense. I find it as difficult to conceive such a substance as an immaterial substance capable of transmitting energy. But I am profoundly conscious of the difference between the limits of our powers of conception and the limits of possibility.

Athenæum Club, April 9 R. C. NICHOLS

* The coefficient of the increase of pressure, the volume remaining constant, as well as the coefficient of expansion properly so called, is termed the coefficient of expansion by Regnault. In view, however, of the variation which exists from the law of Boyle and Marriotte, it is necessary to observe the distinction.